

Field Techniques in Ethnomusicology: The Basongye (Republic of the Congo)

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It is only recently that ethnomusicology has become aware of problems of its internal organization and approach, as opposed to those which arise in connection with the data it has gathered and subjected to analysis. Thus a series of articles in the journal *Ethnomusicology*, running from 1956 through 1962, emphasized the problem of definition of the term “ethnomusicology,” and discussion, to a lesser extent of what ethnomusicology does and how it does it. At the Seventh Annual Meeting of the Society for Ethnomusicology in November and December of 1962, a plenary session was devoted to the purposes of ethnomusicology. And now this conference is directed toward the extremely important question of field technique in ethnomusicology. These are all pressing problems, and the fact of their increasing discussion indicates a growing maturity in the discipline symptomatic of a healthy dissatisfaction with the lack of theoretical knowledge as well as of factual data. Ethnomusicology has reached the point at which it must sit down and examine itself if it is to fulfill the potential predicted for it by the earliest scholars in the discipline.

I find it necessary to make one distinction at the outset, and this concerns the difference between field technique and field method. “Technique” for me refers to the details of data-gathering in the field; that is, such questions as the proper use of informants, the establishment of rapport, the importance of vacation periods both for the investigator and for those he investigates, and so forth. Field method, on the other hand, is much broader in its scope, encompassing the major theoretical bases through which field

technique is oriented. While the two are inseparable in the actual pursuit of a field problem, they are quite different when one is considering the formulation of that problem: technique, then, refers to the day-by-day solution of the problem of data-gathering, while method encompasses both these techniques and the much wider variety of problems involved in creating the framework for field research.

I must confess, with some apology, that I am not quite clear as to whether this conference has been called to consider field method or field technique. Both in telephone conversation and correspondence, the words “field method” have been stressed, but the content of these communications has been concerned with “field techniques.” I am making the assumption, therefore—I hope correctly—that it is field technique which is under discussion here, and not the broader problems of field method. At the same time, since it seems to me that technique cannot be discussed significantly without a prior consideration of method, it seems necessary to make at least some general remarks concerning the latter.

I feel rather strongly that ethnomusicology in the past has given too much of its attention to data-gathering without suitable consideration of the framework within which data-gathering must inevitably be couched. This is emphasized in the literature of the discipline which tends to be devoted in greatest part to the analysis of the sounds of music without reference to their cultural matrix, to the description of music instruments as physical forms, and very seldom indeed to what music is and does in human society.



Ethnomusicology to this point has tended strongly to be concerned primarily with descriptive materials and studies; it has emphasized the “what” of music sound rather than the broader question of “why” and “how.” This, of course, has not been ethnomusicology’s exclusive preoccupation, but it has taken a primary position in our studies.

Thus is precisely that we tend to stress field techniques rather than the total context out of which these techniques arise, for techniques are concerned with data-gathering, while method goes much more deeply into the reasons which lie behind data-gathering and the formulations of problems and hypotheses which validate the kinds of techniques we use. Field method, then, involves much more than the techniques of gathering data, for before data can be gathered—unless the study is entirely descriptive and devoid of other aims—the investigator must face the fundamental problems of hypothesis, field problems, research design, and in all of these, the relevance of theory to method.

To a considerable extent, it is the last—the relevance of theory to method—which is the most important and perhaps the least understood, for no problem can be designed in terms of basic hypotheses without consideration of theory. Herskovits has expressed this with great cogency in noting “...the fact that the conceptual scheme of the student deeply influences not only the execution of a given field problem but also the way in which it is formulated and planned” (1954: 3).

A number of examples of the relevance of theory to execution can be given. Is the aim of the study to record and to analyze music sound, or is it to understand music in the context of human behavior? If the former point of view is taken, then the primary orientation of the field worker will be toward recording an adequate sample of music sound in order that this may be returned to the laboratory for analysis. If the latter is the aim, then the field worker becomes almost automatically an anthropologist, for his concern is not more upon the recorded sample than it is upon the much broader questions of the use and function of music, the role and status of musicians, the

concepts which lie behind music behavior, and other similar questions.

Again, there will be a considerable difference both between the planning and the approach of the student who wishes to make a music survey of a broad area, as opposed to him who wishes to make an intensive depth study of music in a single culture.

Or a substantial distinction will be found between the methods and techniques of the scholar who is pursuing his research for the sake of expanding knowledge in general, and the scholar who is attempting to solve questions the solution to which are to be used in a practical, applied way.

These three brief examples indicate that the orientation of the investigator has a major effect on the approach used in fieldwork. The relationship among theory, conceptual framework, method, and technique is a close one; no fieldwork exists in a vacuum. This does not imply that the field worker can simply fall back upon his general orientation and rely upon it to take care automatically of his research project. To the contrary, any such project requires the careful delimitation of problem, hypothesis, and research design, each of which, in turn, will be shaped by the underlying theoretical orientation of the student.

Again I must apologize for such a lengthy introduction to remarks upon field techniques. Yet it seems vital to me that we understand that field technique does not operate in a vacuum; rather, it is shaped, and even to an extent limited, by the organization of the entire field project, which in turn is a product of the student’s own theoretical orientation. This is why amateurism in any field of study is such a difficult problem. We do not resent the amateur because he is amateur; rather, it is because he brings nothing, or very little, to what he studies. And if one approaches a field without bringing anything to it, he goes away empty-handed. Ethnomusicology has done its share of suffering over the problem, and probably for two reasons. In the first place, the collection of music appears to be a simple matter—one obtains a tape recorder, takes it to the field, and in effect, turns it on—presto, chango, he is an ethnomusicologist. But second, much of the work in our field appears

to have been done almost that simply and with no greater aim than to gather music sound facts.

In any case, let me now turn to the Basongye.

I

The Basongye are a group of agriculturalists and hunters who live in the eastern Kasai Province of the Republic of the Congo (formerly the Belgian Congo). They have apparently been in their present location since the 15th century when they were the founders of the First Baluba Empire. Dispersed by the Arab slave wars in the 1880s, they regrouped themselves about 1900 into three major divisions—the so-called “pure” Basongye in the east, the Bekalabwe in the South, and the Bala in the northwest; it is the last group which came to form the basis for my study.

Late in 1958, I received a grant from the National Science Foundation, later augmented by funds from the Belgian American Educational Foundation, and, to Mrs. Merriam, the Program of African Studies of Northwestern University, to carry out field research in the Mayombe region of the Congo. Early in 1959, however, political difficulties began to emerge in the Lower Congo area, and since my wife and two small daughters were to accompany me, I felt it expedient to change the research area. After considerable study, I chose the Basongye, and the NSF approved the change. It may be apropos here to note that though I have spent over a year in the Congo on two separate occasions, and though in each case the basic grant was made for study in the Mayombe, that region remains to this day the only one in the Congo which I have never visited. The field worker’s resilience must be considerable, and indeed, this personal experience of having to change plans completely within a short period of time is echoed and re-echoed in the experience of other anthropologists.

Although they can be stated only in the most abbreviated form, my work was organized in the following terms:

Problem: To understand Basongye music both as sound and as human behavior

Hypothesis: That the understanding of music (both as sound and as human behavior)

depends upon the understanding of the total cultural context in which the music is found.

Research design: The intention was to spend fifteen months in the field, though the grant itself was for one year. The first ten months of this period were to be spent in a single village concentrating upon general ethnography, but with constant attention given to music, and to gathering both broad and specific data. The three following months were to be given to intensive study of music alone, based upon the ethnographic information gathered by that time. The last two months were scheduled for spot-checking the data for the village against the situation in other neighboring villages in order to obtain an idea of the degree to which my information could be generalized for the Bala and, hopefully, for the Basongye as a whole.

Theoretical orientation: While this is obviously too complex to detail, my theoretical orientation comes out of cultural anthropology and most specifically, from the teaching of Melville J. Herskovits.

It is obvious, then, that the techniques used were those of cultural anthropology, and although they cannot all be discussed, it seems worthwhile to mention some of them, particularly since every field experience adds some new techniques hitherto unknown to the investigator.

II

There is no question, of course, about the advisability of reading everything possible about the people to be studied before pursuing the actual field enquiry. In this case, however, the literature extent was rather meager, and but two monograph-like works existed. The earliest, published in 1908 (van Overbergh, 1908), is a compendium of “facts” about the Basongye, culled from travelers’ reports, missionary accounts, and the like, and as such, it was of limited use. The second, published in 1922, (Torday and Joyce, 1922), is a twenty-eight page “monograph” based on material collected from a small number of informants some ten to fifteen years earlier. In addition to these two works, there is a number of

articles of varying length and merit, most of which are devoted to Basongye visual art. For the first time in my own experience, I took portions, at least, of these works to the field with me. Thus I had relevant parts of the van Overbergh the entire materials of Torday and Joyce, and a number of articles, and I found this an extremely useful technique. Since neither of the “major” works concerned the Bala specifically—van Overberg applies most particularly to the “pure” Basongye, and Torday and Joyce probably to the northern Beneki or perhaps to the Basanga sub-tribe—I found I had comparative materials, both of ethnography and of time. Further, this material gave me a number of leads in that the groups they described paralleled, at least, the organization of the Bala.

Perhaps the most frequently discussed question among students who have not yet done field work is that of making initial contact in the field, and yet it seems to me that this is one of the simplest problems of field research. I have used a number of techniques in this respect, but those employed among the Basongye are perhaps as typical as any.

The first problem concerned finding the proper place to work. The sources showed clearly that the Basongye had moved about since first contact was made by a Westerner in 1882 (von Wissman 1891), though apparently within a fairly circumscribed area marked roughly by the towns of Kabinda on the south, Sentery on the east, and Tshofa and Pania-Mutombo on the north and west. Entering the area from Luluabourg, I stopped in the town of Lusambo, somewhat to the north of the major Basongye territory and, after spending a day there, moved on to Kabinda which subsequently became my central base of operations. At Kabinda, through contacts made earlier in Belgium, I obtained the services of an interpreter, Mulenda Arthur, who turned out to be a sensitive, intelligent, and generally excellent interpreter, and who stayed with me throughout the year, and we began a systematic survey of the Basongye area. What I was looking for was a sense of the area in general, of settlement patterns, of concentration, if any, of music activity, and of what I might consider to be a “typical” Basongye village. At the same time, practical considerations

enters; there must be a place for my family, and because it soon became apparent that food would be a problem, there must be accessible supply lines, and so forth.

To these ends, Mulenda and I spent ten days traveling about a thousand miles through Basongye country, stopping at approximately one hundred and twenty five villages, of which we intensively surveyed perhaps thirty five. There followed a period of two days during which Mulenda and I compared our notes and impressions, checked with the Belgian administration, which, of course, had the final say, and determined on a single village, Lupupa. Our reasons were many: it seemed to be approximately “typical” in terms of size and location; music activity was apparently normal; there was a mud-and-thatch rest-house which could be repaired and would be suitable for minimal comfort for the family; the village was only some twenty five miles from the nearest administrative post and arrangements could be made to have food brought here from Kabinda, some one hundred sixty miles away; it was the interpreter’s village, and there were a number of advantages in that.

Once we had settled on Lupupa, and been granted permission to work there, we re-visited the village and called a meeting of *bamwilo*, or notables, at which I explained as honestly and in as much detail as I could, what I wished to do over the coming year. I then said that I would be pleased if they wished to have me and my family, but that if they did not, I would not stay; and left the village again, announcing that I would return in three days to hear their decision. In truth, I am not sure how free they were to make a decision, for this country at the time was still much in the bush and to turn down a white man may well have been impossible in their terms—but at least I hope they felt they had some say in the matter, and when we returned in three days they offered us their hospitality. Perhaps it should be added that I never regretted the decision to choose Lupupa, for it did turn out to be probably as “typical” as any village we might have chosen, and we found the people warm and pleasant friends.

Once the initial contact has been made, the problem of establishing rapport is not by any

means always an easy one. People remain suspicious of the investigator's motives, and his actions are closely watched and weighed. Although one can give gifts, or indicate his intentions in other ways, what seems vital to me is that over time he must remain honest to his purpose and convince the people that he is truly interested in their way of life and even dedicated to it. Until such time as he can do this, he is suspect and his rapport cannot be as good as he wishes it to be. In our own case, I think five things enabled us to establish rapport, although these took time. The visible arrival of my wife and children was an enormous first step in this direction. Second, I *was* interested in them and their way of life, and it was obvious. Indeed, it shortly became a village joke that I was a man who insisted on being shown and who was not satisfied with words if I could possibly see and do something myself. There was pleasure in the joke, for I am quite sure that my insistence on being shown was regarded as flattering. A third, and extremely important point in the establishment of rapport, was the simple fact that I shook hands with people. I take no credit for this, for it simply did not occur to me not to shake hands. But the Basongye felt that the Belgians did not do this, and indeed, after I came to know them better, they discussed it with me with such bitterness and complaint. Fourth, I announced very early in my stay, and to each informant, that what was said to me was said in strictest confidence and that it would never be repeated to others in the village. I swore Mulenda to this, too, and we were both tested on it in a variety of ways. Finally, we deliberately took a vacation from the village somewhere near the middle of my stay. This was both for our own benefit and for that of the village, and when we returned from that vacation, our rapport was fully established. Our friends came up to us, saying with real pleasure that they had not really expected us to return, and the talking drums boomed out to the neighboring villages: "Our Europeans have returned." Through those and other techniques, we were able to establish, I think, excellent rapport, and the proof of the pudding was that the work went well all year and that it came to be general knowledge in the village that my *kikudi*, or spirit, was undoubtedly not only

Basongye, but specifically Bala, and in all probability from Lupupa itself—The Basongye believe in reincarnation.

The problem of language is a difficult one. We used, as I have said, an interpreter with whom we spoke French—in reality, as our relationship developed, Mulenda and I made up our own language, borrowing words from French, Kisongye, Kiswahili, English, and even occasionally Chiluba and Otetels, which expressed precisely what we wished to convey to each other. I never learned Kisongye, though I came to understand it fairly well and could string together a group of words which convulsed the Basongye, but which they could understand. There is no question in my mind but that the kinds of things I wished to know would have been better understood if I had spoken fluent Kisongye, but practical problems prevented it. In the first place, there is no Kisongye grammar in existence, and the one "dictionary" is really a comparative word list of five local languages and French, and very sketchily done, though it does have some grammatical notes (Anon, n.d.). More important, however, is the limitation of time which constantly plagues American anthropologists who work in Africa. Grants are made for one year, at best, eighteen months, and the time to learn a language in the field must inevitably be subtracted from the total time to do research. In any case, I did not and do not feel that our work suffered drastically from lack of language knowledge, though the finest nuances of detail may have been lost.

Since Malinowski first suggested it in his monumental study of the Trobriand Islanders published in 1922 (Malinowski 1950), most controversy has reigned in anthropology over the extent to which the investigator can and should be a participant observer in the affairs of the society which he is studying. My own belief, based on four field trips, is that participation is possible and fruitful only to a certain extent. I do not know how others feel, but no matter how hard I might try, I cannot pass myself off as a Musongye, and therefore it is impossible to be a full participant. Moreover, participant observation has its distinct disadvantages; for example, one tends to become associated with a certain segment or even lineage

or family, and is called upon to take that group's side in any dispute which may arise—this can definitely be embarrassing and limiting. On the other hand, of course, participant observation leads to a deep knowledge of the intimate details of life, which, in some respects, cannot be achieved in any other way. The answer lies somewhere between the two extremes. I tried to participate wherever it seemed possible and desirable, and was even insistent to a certain degree when the matter was particularly important, but the Basongye always raised barriers against me, and this after all is to be expected and perhaps even to be admired. In any case, we used techniques of direct, indirect, and participant observation, as well as informant use.

Problems of informants are well known and need no further detailing here, save in one respect. I had not realized before working with the Basongye to what extent particular individuals are useful as informants in different ways. Some informants can be used only to corroborate or deny information given by others. Some are excellent at working in detail; others can only give a picture in the broadest strokes. In the course of a two-hour session with Kingkumba, for example, my notes reveal that we touched upon the following subjects: the *lukunga*, a political dignitary; crime; the system of fines; suicide; salt; paths and traditional routes; money; measurements of length and distance; and number. None of these subjects were explored in detail, for that is not how Kingkumba liked to work, but the same notes reveal that within the next few days I had reworked all these subjects with informants whose minds ran to exploring a topic in more depth. There are many other problems involved in using informants, but these have been detailed elsewhere and I shall not go into them here (Herskovits 1954; Paul 1953).

In general, I was most satisfied with the application of problem and hypothesis in the Basongye research. Least satisfactory was the working out of the research design, but this was a matter entirely beyond my control since it involved political events outside the research problem which cut down the length of the stay in the field.

III

I should like to turn at this point to four special problems of methodology and technique, which I feel have been neglected in ethnomusicology though they are of special importance to it.

The first of these is the question of what constitutes the ethnographic or, in this case, more properly, the ethnomusicological truth. This refers, of course, to the fact that within any given culture there is what often seems to be an almost infinite variety in the details of any one kind of behavior or belief. Given the fact that the investigator cannot possibly consider every minute variation, how can he ever know what the "proper" or the "correct" is? In anthropological fieldwork, it has been clear that as better and better rapport is established, there are fewer and fewer misrepresentations, and the informants themselves sometimes correct previous misstatements. This was surely the case with the Basongye. But more than this, there are at least three principal ways in which a check can be made on the reliability of data gathered. First, the statements of informants can be checked against the direct observation of the investigator. Second, information can be cross-checked with other informants. Third, every field worker finds that as his own knowledge of the culture increases, he can spot inaccuracies in informants' statements in terms of internal inconsistency, or as these seem suspicious in terms of other information he possesses.

But what does constitute the ethnomusicological truth? This is a question not often considered by ethnomusicologist, although it has surely been taken into consideration by such field works as Helen Roberts in her study of variation in Jamaican folk song (1925). Perhaps the most interesting single answer has been given by John Blacking (1959) who suggests that, as the anthropologist must generalize his information from a mass of individual statements, so can the ethnomusicologist generalize different variations of the same song. There is much obvious material for argument here, but the suggestion is an intriguing one and the basic problem called for much further consideration.

The second question concerns the technique of spot-checking, an aspect of method which to the best of my knowledge has not been fully exploited by ethnomusicologists. The spot study refers to the checks, made usually toward the end of the field stay, of the validity of the data gathered as it applies to a wider context. Thus in a small, undifferentiated society such as that of the Basongye, the investigator may spend the bulk of his time in one village, but in the last few weeks travel to neighboring villages to check his information with other informants. Although this formed a definite part of our research design, the portion of our Congo stay which was to be devoted to spot-checking was the summer of 1960; political developments made this patently impossible. However, the importance of the technique for ethnomusicology is considerable, for in most studies, the investigator tends to be able to speak only for the restricted area in which he has worked, and it is impossible to ascertain how widely the patterns of which he speaks can be generalized.

The third special technique of importance to ethnomusicology concerns the matter of restudies, a methodological resource which has been utilized very rarely indeed in our discipline. Oscar Lewis (1953: 466-72) has distinguished four types of restudies in anthropology all of which are equally applicable to ethnomusicology. While there are several reasons for the lack of restudies in ethnomusicology, such as lack of personnel and the like, it seems clear that we have reached a point at which this technique would be extremely important and revealing. The work of Hornbostel, Herzog, Densmore, and others now has a substantial time depth, and all stand as baselines from which restudies could make significant contributions both to methodology and music change.

Finally, although it lies more in the realm of analysis of materials gathered than in the question of field technique, mention must be made of the comparative method, for if comparison is to be an aim of analysis, it must be considered in the research design. We have paid far too little attention to this aspect of our study, and the result

is that materials from different investigators are often not comparable.

IV

I have tried to discuss in this paper three different aspects of method and technique in ethnomusicological fieldwork. Of these, the most important in my view is the too-often unrecognized interrelationship between theory and methodology, and I should like to reemphasize two points in this connection. The first is simply that fieldwork is not done in a vacuum; the field worker always operates out of a theoretical background which shapes his work. This fact has gone virtually unrecognized in ethnomusicology, and in my opinion, we need to give a great deal more attention that we have to a clarification of the aims of our discipline. The second point is perhaps only an extension of the first, but it is that ethnomusicology has been largely concerned in the past with data-gathering per se, and not with an understanding of that data. The result is that we have emphasized the "what" questions, but not the "hows" and "whys" of music.

My second concern in this paper has been with some specific field techniques among the Basongye, and my third with some special techniques which seem to me to be of particular concern to ethnomusicology. I think it is well that we are beginning to consider questions of methodology in ethnomusicology; there are many such problems before us.

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Discussion Following Alan Merriam's Paper

I. Conceptual Frameworks

Malm: By theory in this case you mean the conceptual framework, don't you?

Merriam: Yes, and with all that that implies that lies behind it. If I were a British social anthropologist, I would do very different kinds of things than what I do being an American cultural anthropologist.

Look back over the *Journal* and over the writings in the history of ethnomusicology, and I think you will find that what ethnomusicology has done primarily is data gathering without reference to a theoretical framework. I think this is pretty clear. It seems to me that it is, in any case. Is there in your field, historical musicology, for the sake of argument, theory which differs from one group to another group? This I am asking as a flat question.

Malm: Different frameworks in which people approach history? Absolutely.

Merriam: O.K. Fine. I would assume there was. But I think you and Robert Garfias have never talked about the framework out of which you operate. How does this affect the kind of study that you do on Japanese music compared to the kind of study Robert does on Japanese Music?

Malm: I think it enriches it, if anything.

Merriam: Of course it enriches it, but it means that you are coming out of a different set of concepts, which is inevitably going to affect the work that you do and change the kinds of things that you are looking for. Now maybe this isn't as strong in historical musicology as it is in anthropology. It is enormously strong for us. What I'm trying to say is that we work out of different theoretical backgrounds which will affect the kinds of things we do and, therefore, the results of what we do. We have all got to know what these backgrounds are, before we can bring them together.

Seeger: The theoretical background bristles out of everything. You find it in practically everything. I think the thing we must remember is that we are comparatively young as an inter-disciplinary venture. What we are trying to do now is to build a theory for the discipline, for the study. Now naturally, just as the ordinary song in oral tradition is known in many variants and doesn't have any existence outside its variants, so the theory of ethnomusicology as a group undertaking will only be known in the theory that the individual workers carry. But we must establish a certain agreement, a certain consensus among the

competent students, before our study can be accepted, so to speak, as an established mature study.

Merriam: And for our own benefit of understanding each other.

Seeger: When we look for the first time over the field work of some worker and, as I say, its theory bristles out of almost every sentence, one of the things we look for is conformance to what consensus there is among us of what objectives, what the standards and the criteria of the study are. Also, we look for new things, for modifications of the theory, because the theory grows, just as the things we are studying grow. And it's always a moving thing; it is never a static thing. As soon as it gets static, then it begins to die. Now we are building our theory and we have had some very nice statements of theory here. It's a particularly comprehensive one. I don't know that everyone here will accept it.

Merriam: I don't expect everyone to accept it.

Seeger: You don't expect everyone to. But what we are looking for is this agreement, eventually more and more agreement. If Einstein tries to make a unified field theory of the universe, he is addressing himself to this consensus among physicists. If everybody has a different theory, he can't do anything. But he was one of those men in whose work you looked for the qualities and the features that would advance the theory to a higher level of development, and that is exactly the same situation that we are in now. Everyone who goes out into the field either doesn't meet the levels which are already established, or he more or less floats along on the norms as they are and does a good craftsman-like job, or else he advances that theory. It's plus, equal or minus right down the line. That is what we are all looking for here. We hope that no one goes in the field and doesn't come up to the norms of the study. We regret the amateur for that reason but the least we expect is that he will reach the norm which now is more or less agreed upon and we hope that he'll add something to it. Our final aim is to advance the study, not just to keep it where it is.

Powers: Materials from different investigators often are not comparable. I couldn't agree more, and I think this is the most important thing that we have to do, to try to find a way so that, when I get through what I am doing, you can use it if you need it.

Merriam: I find really even with the material that should be most comparable—David's—we have approached things in very different ways and it is very hard to make a comparison.

Seeger: But that will diminish as time goes on.

Merriam: Of course, but what I am saying is that we have to be much more cognizant of this as a problem.

Hood: I would like once again to bring in this note of caution which I tried to start with the first day. I think we have to be careful that we don't simply take a theory of anthropological application or musicological application as ethnomusicology. We have to find our own way, musicological or anthropological, an in-between here.

Merriam: Will you go along with me to the extent, also, that what we do analytically depends upon theoretical orientation?

Hood: Yes, but I think we must be careful here. I believe the material is going to shape that theoretical orientation. A man in the past went out in the field with the theory, determined he was going to prove it. And this is wrong. If you go out into the field bristling with theories, you are going to come back proving them.

Merriam: There is a distinction between theory and theories. What we've done in ethnomusicology, for example, from the anthropological viewpoint is we had a whole era in which our theory was an evolutionary theory, and then we had a whole era in which we had a *kultur restoresche schule* idea with the cultural circles, etc. And the functional theory has come over to us, etc. These are theories. This isn't really what I am talking about. I am talking about theory. This is a broader kind of proposition, you see. A specific theory, if you wish, like *kultur restoresche schule*, this certainly did compress your materials. They went out looking for things that would prove this kind of

business. That we throw out, I hope. On the other hand, I have to come with a theoretical background, because I am trained in a certain way as a certain kind of anthropologist.

Seeger: That is just what I was trying to say in different words when I first started in a moment ago. The theory is the consensus of ethnomusicology. This is not to deny the dangers of going around and forcing everything into it. A good theory will be on guard against that.

Hood: Well, if you mean to say a basic theory, and the underlying theory is not to have theory, then I will agree with you.

Merriam: Now what I think Charles has said is that what we must do is to create out of our own theoretical orientations an approach which will give us mutually compatible results. We are close now, but we are not precise.

Hood: I want to state and restate and stand on the table and scream about it. I say that the people here who are trained in musicology had better not bring the disciplines and techniques of musicology to what we call ethnomusicology. Because if they do, it is just going to be as junky and bad as it has been in the past. My assumption is, and you are helping reinforce it, that what is true in connection for musicology in relation to ethnomusicology is likely to be true for cultural anthropology in relation to ethnomusicology. And I was suggesting that whatever your theoretical orientation as cultural anthropologists, they probably need an overhauling if you get involved in ethnomusicology.

McAllester: But I don't think we need as much of an overhaul of our theory as you do.

Hood: I agree that our challenge is much greater. I am just suggesting that anthropological theory per se is probably not adequate for this job.

Merriam: I don't think there is any argument.

McAllester: The fact that we keep coming together and feel a need for each other, I think is extremely hopeful. That is, we're not so far away that our tendency is to split off and say, the heck with those people, I don't get any

communication at all. So, I don't feel like throwing up my hands and saying that we have two separate fields.

Seeger: May I make a footnote to that? I don't believe that we have the right to use the term "musicology" in the sense of the historical musicologist. They call it musicology, but it isn't. It is nothing but one possible orientation in musicology, which is still looking backwards. It is devoted to one area of the world, and only part of the music of that area.

Ethnomusicology is musicology, and that thing which is called musicology is something else, I don't know what it is, but you can give it a word if you want, and it is not a very complimentary one. We are, in ethnomusicology today, really getting at musicology for the first time. Suppose you have a physicist studying astronomy, but he only sees a small quadrant of it. That is, he only studies what can be seen of the heavens from the northern hemisphere. This is what the musicologists have done in comparison with the astronomers. We are trying to study the whole heavens, the way the astronomer does. That's ethnomusicology.

II. Field-Work Problems

Powers: As far as the techniques are concerned, given the differences between Africa and India, what Alan did in Africa is more or less as I did in India. I insisted on doing and being shown, rather than just taking somebody's word for something.

What constitutes ethnomusicological truth? How do you know the proper and correct ways in which things are done? How do you check them? You check the informants against the observation. You hear something, you try to write it down. You sing it for somebody and he tells you whether you did it right or not. You check the informants against each other. You hear one fellow sing a song, you hear the song sung by a number of different people and you get, so to speak, a generalized version of the different versions of this song as a Platonic ideal in your mind, and you know comparatives. Soon you are checking the

informants against your own growing knowledge of the practice. Well, I can certainly beat that horse for two days. As far as spot checking over a wider area is concerned, my own work was in South Indian classical music, but I found, needless to say, that this pushed me north, although not to the extent that I would like to continue it.

McAllester: Harry talked about proper and correct ways of performance, and it just flashed through my mind that, as an ethnologist, any performance by the native people that I am working with is equally valuable. Of course, I want to know what they think is the better performance, but I have to abandon certain European attitudes as to what is good performance and what is bad performance. That is, you find yourself working in places where right and wrong performances are very differently defined.

Garfias: Allan Merriam pointed out something earlier that struck me very strongly. He chose a village that was musically average. I think the musical half of us would choose one that was musically abnormal, special, and outstanding. I think without question, we would choose the one that had the most musical activity and the greatest variety.

Malm: If you were going to Indonesia, would you avoid Jakarta, and so on, and go to the average village?

Merriam: I probably would, yes, I think that is probably, again, my orientation. I would rather ask, "What are the bulk of people doing?"

Garfias: I personally would love to have someone like you and David go to Japan and look at it, and see what you come up with. I am sure it would be different. I would love to see what you would do with a situation like that.

Hood: One could make a study in central Java of the medium and small villages and gamelan therein. It depends on an objective. If you want to understand what gamelan practice involves, my orientation was to go where it is found in its very best form. You start with the palace. Today you also start with the radio

stations. These are your finest musicians. The tradition is at its zenith here. If you can comprehend what is happening there, you find that as you move outside the palace and radio station, just next door where there is a wealthy Batik merchant who has a beautiful gamelan, you are able to appreciate the fact that these fellows out here are just not quite as good, that's all. The same principles are involved. You go on another step and get a fairly cheap grade of bronze—there are three grades—and then you get to the brass, these are really material differences. Finally to a little iron gamelan out in the village. With that orientation you find that what is going on in the village you understand quite well, the limitations that they are working under, and why it's that way.

Merriam: But I would hope that at least I am a good enough anthropologist that, having done the village, I would see this and inevitably work out.

Hood: As long as it's done, I think it is easier to do it the other way.

McAllester: The village here is "folk culture," in Redfield's terms, that is sharing many aspects of a civilized culture on which depends this music. For example, they play gamelan orchestra, which is related to a very highly sophisticated one, like a hillbilly band is related to a symphony orchestra in some distant way. You have to know the sophisticated center to understand "folk culture." It is rather different in tribal cultures where you don't have a great center with lots going on. And so that difference of approach. Allan as an anthropologist might well go to Jakarta first to study folk cultures in Indonesia. He would read the literature, steep himself in the sophisticated culture, knowing that the folk culture is very dependent on it, and much a reflection of it.

Seeger: But can't we accept as an axiom of study that until you get all the music of a culture, you can't talk about the social or cultural function of any one part.

Powers: Naturally, I would agree. But nobody can do everything. To get all the music of a culture like that of India is a pretty tall order.

Seeger: Well, if we cannot do that, then we might as well just fold up and go home. The end of study is not the detail. The end of the study is the relation of the detail to a whole. What is the whole? It's mankind as a whole; it is mankind's place in the universe as a whole. You can't see the forest for the leaves. I am not saying you mustn't study the leaf but you must also study the tree, you must study the forest.

Maceda: Actually, you devoted twelve months to the study of the cultures before you studied the music.

Merriam: It didn't work that way, but that was the design. The first eight months, it worked out this way. They were devoted to the ethnography of the Basongye, all of this with music as the focal points. In other words, it wasn't as if I did eight months without ever thinking about music, then all of a sudden started music. I was always gathering information on music, but I felt for my particular kind of problems, I wanted to know the whole culture as well as I could. The last three months were devoted exclusively to music, so it wasn't a question of either/or. It was a question of going all the way along, and then suddenly generally stopping the ethnographic approach and going into music.

Maceda: Of these eight months, how much would you go into the kinship system rather than analysis of drumming?

Merriam: I went into a kinship system far more in those eight months than I went into an analysis of drumming.

Maceda: Would this have any bearing on drumming, or on some aspect of music?

Merriam: Yes, definitely, so far as I am concerned. How music is being used in terms of the social system. If I don't know what the social system is, I can't relate the two.

Maceda: I understand that. That's still too general.

Powers: When you are doing the anthropological work with the Basongye, the point is you have to do it, because no one else has done it.

Whereas, when we go in some place, we have a tremendous amount of work that has already been done and taken care of by other people, including people in the culture itself.

III. Historical Studies

Seeger: There is a difference between the historical and the systematic approaches to music. The historians study what was, and they try to pretend that they have to know that before they can know what is. It is the exact opposite of that. You've got to know what is first, then you might make some guess about what was. You only know the past in terms of the present.

Merriam: It seems to me that the important thing in ethnomusicology is that maybe fifty years from now, the people who supersede us are really going to have some good material to work with, hopefully. They can begin really historical studies, although I think we have neglected this. I think we are ready now to go back to Micronesia, and redo what Hertzog did, for example. I think this would be extremely fruitful.

Kishibe: We have a distinction between historical method and theoretical or anthropological method. Don't you think so?

Merriam: I would be reluctant to make that sharp a distinction. Although I personally think there is a distinction. The two are so melded together in anthropology. For example, in working with the Flathead Indians, it is of extreme concern to me. I am absolutely delighted that I can go back and get good information on music back to 1806 when Lewis and Clark made the first contact. I think this is terribly important, and it will be part of the study that I do.

Hood: The historical orientation in this field has been grossly neglected, and I think it is a terribly important one. I think we have a whopping advantage over the study of European art tradition, historically, because in many, many societies we have the living anachronisms with which we may work comparatively and get historical depth. To a

degree, it's reconstruction but it is far more hazardous when you have a dead manuscript rather than a living anachronism. *Gagaku* is a beautiful example of long continuity. In Java, there are a good many too. I think we have such a beautiful potential for historical depth, and few of us have paid any attention to it.